The attenuation factor $r^{-\frac{1}{2}}$ in (1) does not count as distortion.

The wave may go either way, and various cases can be elaborated. If the wave is outward, the axis ($r=0$) is the source. The plane $\theta=0$ is a perfect electric conductor. The electrification is of the same sign on its two sides. Other details may be got from the formulæ.

I give an example to show the not very obvious electrical meaning. Let the infinite plane conductor with the straight edge be one pole of a condenser, and a straight wire placed parallel to the edge, and close to it, be the other pole. Join them by a battery, charging the plate and the wire. Bring the wire right up to the edge, and reduce its magnitude to a mere line. (This is to be done in order to attain the ideal simplicity of the formulæ.) Take away the battery. Then the electric field is given by

$$c v \mathrm{E}_1 = -\frac{\sin \frac{1}{2}\theta}{2 r^{\frac{1}{2}}} f_0, \qquad c v \mathrm{E}_2 = \frac{\cos \frac{1}{2}\theta}{2 r^{\frac{3}{2}}} f_0, \qquad (3)$$

where f_0 is a constant and c is the permittivity.

Finally, discharge the condenser by contact between edge and wire. Then the result at time t later is that outside the cylinder of radius $r=vt$ the above field (3) persists, whilst inside the cylinder there is no **E** or **H**. An electromagnetic wave separates these regions. It started from the axis at the moment of contact, and as it expands swallows up the whole energy of the field, and carries it to infinity. Similarly, as regards the charging of the plate, only the "battery" should, to have the same formulæ, be an impressed force acting at the axis, between the edge and the wire. At time t after contact, the electric field is established fully within the cylinder $r=vt$. On its boundary is the impulsive wave which is laying down the remainder. It also, if the contact be instantaneous, wastes an equal amount of energy at infinity.

Similarly, by varying the impressed voltage anyhow with the time, the emission of an arbitrary wave of **H** results. With a real plate and real wire, the main features would no doubt be the same. The use of the line wire introduces infinite voltage.

What somewhat disguises the electromagnetics is the existence of the steady electric force, or parts thereof, along with the electromagnetic **E** and **H**, particularly when f is arbitrary. There is a similar complication in the spherical wave when the total electrification in any thin shell is not zero. There is then an auxiliary internal or external electric force to make continuity.

We cannot have an undistorted wave from a simple line source. But in the example the apparent line source will be found to be a doublet. For the curl of **e** (impressed force) is the source of the wave. It is double, positive on one side, negative on the other.

Solutions of the type

$$\mathrm{H} = \sum \frac{\mathrm{A} r^n \cos(n\theta + a)}{(v^2 t^2 - r^2)^{n+\frac{1}{2}}} \qquad (4)$$

or the same with r and vt interchanged in the denominator, are not distortionless, save for the solitary term in which $n=-\frac{1}{2}$. The above distortionless cylindrical wave (1) is unique. Prove by the characteristic.

April 29. OLIVER HEAVISIDE.

Seismometry and Gēite.

UNDER the above heading Prof. J. Milne contributed an interesting article to NATURE of April 9, p. 538, on which I wish to offer some remarks. Prof. Milne seems hardly to realise the significance of the enormous pressures to which the earth's deep-seated material is presumably exposed. One of his objections to the hypothesis of an iron core seems to be that the wave velocities for an infinite isotropic medium of the density and elasticity of iron do not accord with the velocities of earthquake waves. This objection, however, is not conclusive. In an infinite isotropic medium there are *two* purely elastic wave velocities, v_1 and v_2, given by the equations

$$v_1 = \sqrt{(m+n)/\rho}, \quad v_2 = \sqrt{n/\rho},$$

where ρ is the density, m and n Thomson and Tait's two elastic constants. On the ordinary theory, n/m may possess any value consistent with Poisson's ratio γ, or $(m-n)/2m$, lying between 0 and 0·5. Six years ago I showed (*Phil. Mag.*, March, 1897, p. 199) that observed seismic wave velocities can be accounted for by elastic waves without postulating any abnormal value for Young's modulus—the modulus to which Prof. Milne repeatedly refers. For instance, we get values of 12·5 and 2·5 kilometres per second respectively for v_1 and v_2 in a medium of density 5·5 with a Young's modulus of only 10^9 grammes weight per sq. cm., if we suppose $n/m=1/24$, or $\gamma=0{\cdot}48$ approximately; and the same results follow if we increase density and elastic constants in the same proportion.

In iron, as we know it, γ, of course, is not 0·48, but more nearly 0·25. A material, however, which under low pressures has $\gamma=0{\cdot}25$, *may*, after prolonged exposure to enormous pressures, behave as an elastic medium with γ very nearly 0·5. In fact, if the deep-seated material acts as an elastic medium, the only consistent way yet pointed out for its doing so is by its behaving as if γ were very near the limiting value answering to incompressibility. Neither of the elastic wave velocities, it should be noticed, has anything directly to do with Young's modulus, a point which cannot be too clearly emphasised. Another consideration is the possibly appreciable influence of gravity on the wave velocities.

Coming now to the question of the behaviour of magnetographs at times of seismic disturbance, there must undoubtedly be magnetic disturbances occasioned by earthquakes in more than one way. When a violent earthquake occurs where magnetic material abounds, there may be a vast movement of magnetised matter; there may be a great change in the stresses throughout adjacent magnetic material; and there may be a great change of local temperature. Any one of these causes will give rise to a magnetic disturbance which should be practically simultaneous all over the world, and should *precede* any seismic movement at distant stations. It should also diminish very rapidly as the distance from the earthquake origin increases.

Again, as the seismic waves travel out from their source they must cross volumes of magnetic matter, and the mechanical effect on any such volume must necessarily produce changes in its magnetic field. Owing to the finite velocity of seismic waves, the displacements and stresses simultaneously existent in different parts of any large magnetic volume must be in all kinds of phases, leading to considerable interference between the magnetic disturbances to which the different parts give rise at any considerable distance. Thus the most plausible explanation of why a magnetic disturbance of some prominence—if real—should appear at one observatory, but not at another only 100 miles off, is certainly the existence of magnetic material close to the former. Supposing that such local material exists, the magnetic phenomena may be expected to vary according to the direction in which the earthquake wave is travelling.

One of the chief difficulties in reaching definite conclusions is the contracted time scale usual in magnetograms. If the true seismic and the apparent magnetic disturbances occur within a few seconds of one another, it is usually practically impossible to say which is the earlier. To see the full force of this, one must remember that a by no means improbable explanation of why apparent magnetic disturbances accompany earthquakes at one station, but not at another, is that the magnets at the former, owing to pattern or site, may be much more sensitive *seismographs* than those at the latter.

Again, it must be remembered that whilst the so-called "large waves"—rather an unfortunate term—produce in general a much greater effect on a horizontal pendulum than do the "preliminary tremors," it by no means follows that the same will be true of either the true magnetic or the purely mechanical effects on a magnet. Much may depend on the method of support and the time of swing.

The passage of the "preliminary tremors" and "large waves" due to an earthquake often occupies several hours, and during this interval several true independent magnetic movements are not at all unlikely to present themselves, even at times of general magnetic calm.

For all these reasons a careful intercomparison is wanted of magnetic and seismic records from a variety of stations. Something might be done by running magnetographs for some time in a district where a local magnetic disturbance

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is known to exist, and contrasting the results with those obtained elsewhere with the same instruments.

Prof. Milne mentions Kew and Greenwich as representatives of stations where magnetic and gravitational anomalies do not exist, but, as a matter of fact, Rücker and Thorpe's magnetic survey does show a small magnetic anomaly in the Thames Valley, and certain foreign observers have also inferred a gravitational anomaly.

As to Prof. Milne's special term "gēite" for material in the earth's interior, I must confess that the application of a new word to the unknown material of a problematical core seems to me more likely to hinder than assist. Such special terms constitute an additional obstacle in the way of those who are not specialists. Also existing terms, such as nucleus and core on the one hand, and layer or crust on the other, seem not inadequate, the context showing whether it is the material that is immediately in view.

I have had repeated occasion to deal with elastic problems involving a core and a layer or layers. In fact, the very "earth" for which Prof. Milne expresses a preference, consisting of a layer of about 1/20 of the earth's radius in thickness with a density of average surface rock, and a core of specific gravity approaching 6, is one which I selected some years ago for the purpose of investigating luni-solar tidal action (*Cambridge Phil. Trans.*, vol. xvi. p. 151). Thus I do not speak without experience.

A final point to be remembered is that, according to the investigations of Gauss and others, the earth itself is a magnet of considerable moment. Any theory which claims even provisional acceptance may be expected to give a plausible explanation of this fact, and of the secular change observed in terrestrial magnetism. C. CHREE.

Photograph of Oscillatory Electric Spark.

THE enclosed photograph of an oscillatory electric spark, like most of those which I have taken, differs in some respects, so far as I have seen, from those which have been recorded by other experimenters. It was obtained by the discharge of 22 square feet of coated surface through ¼ mile of coiled wire, the electrodes being of magnesium. The picture was focused on a circular plate fixed on the end of an electric motor, so as to revolve in its own plane. The number of double oscillations was about 3000 per second. C. J. WATSON.

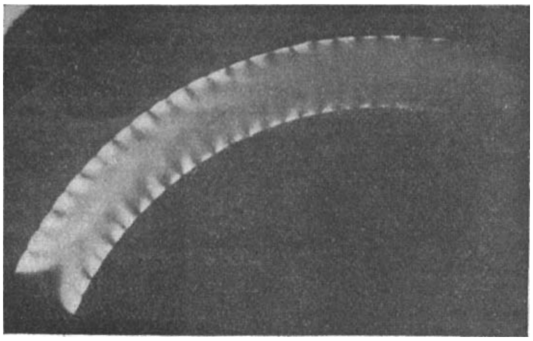

FIG. 1.—Oscillatory electric spark.

Bottville Road, Acocks Green, Birmingham.

Our Rainfall in Relation to Brückner's Cycle.

IN the instructive paper on solar and meteorological changes in NATURE (May 7), I observe that Dr. Lockyer suggests 1913 as probably about the centre of the next wet period. A consideration of barometric changes appears to lead to a similar result, and I may be permitted to recall a letter sent you in 1898 (NATURE, December 22, p. 175), in which, discussing with such data the question, "Where do we stand in Brückner's cycle?" I mentioned 1911 as probably near that centre. Such estimates must, of course, be regarded as merely approximate, and open to revision.

This important cycle of Brückner's was lately discussed in a number of letters to the *Times*, and it is satisfactory to see that more adequate attention is now, though tardily, being given it.

Is it not objectionable to treat the British Isles as a whole, since, on Brückner's theory, the western portion shows opposite variation to the eastern?

There is a method of curve-making which seems to be little used by meteorologists, but which is, I think, to be recommended. A series of annual rainfall figures (say) is first translated into a series of plus and minus values (related to the average), and this series is then added algebraically step by step, *e.g.*

$$+9\cdot3-1\cdot4+0\cdot6+0\cdot9-1\cdot6+1\cdot3, \text{ \&c.}$$
$$+7\cdot9+8\cdot5+9\cdot4+7\cdot8+9\cdot1, \text{ \&c.}$$

This second series is then thrown into curve form. The process is analogous to paying in money to a bank, and drawing money out, and the point reached by the curve at any given time indicates the balance.

Each upward (annual) extension in such a curve represents a wet year, and each downward extension a dry year, and the degree of wetness or dryness is also indicated.

A large comparison of such curves for European and other stations would, I think, throw a good deal of light on the Brückner theory. ALEX. B. MACDOWALL.

The Propagation of Phthisis.

IN a work called "Opera nuova intitulata il Perche, utilissima ad intendere le cagioni de molte cose, &c.," published in Venice in 1520, the following passage occurs:—"Dal sputo del Tisico o da la sua boca viene fuora un vapore fetido e acuto che entra poi per la boca de colui che conversa con quello e corrode simelmente il pulmone de esso e in questo modo genera tisica."

Substitute for *vapore* "material particles," and we have the modern conception of the mode of propagating consumption. This anticipation of modern science seems worthy of note. EDMUND MCCLURE.

TANGANYIKA.[1]

THE title of this work is perhaps somewhat misleading. The reader who expects the book to contain only discussions of speculative questions will be agreeably surprised by finding that the positive contributions made in it to our knowledge of the geology, botany and zoology of Central East Africa are of the most extensive and valuable character. The two expeditions which the author undertook in 1896 and 1899 to Lake Tanganyika and the surrounding districts, following upon the researches of earlier travellers, have thrown a flood of light upon both the geological structure and the fauna and flora of this part of the world, while they have incidentally suggested a number of difficult problems of no small interest alike to the geologist and the biologist.

The surveys of Mr. Moore and of Mr. Malcolm Ferguson, the geologist who accompanied him, have been of value in rectifying and making noteworthy additions to the maps of the area visited. The geographer will find references to a number of new mountain peaks, the heights of many of which are given, with the determination of the heights above sea-level and the depths of many of the lakes, in several of which numerous soundings and dredgings were made.

One of the most valuable results of Mr. Moore's explorations is the confirmation he is able to supply to the conclusions of Mr. Scott Elliot that there exists in East Central Africa a great mountain chain running north and south, and rising at many points, even in this Equatorial region, above the limits of perpetual snow. The height of the snow-line is fixed by Mr.

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[1] "The Tanganyika Problem; an Account of the Researches undertaken Concerning the Existence of Marine Animals in Central Africa." By J. E. S. Moore, F.R.G.S., author of "To the Mountains of the Moon" Pp. xxiii + 371; with 7 maps and 140 illustrations. (London: Hurst and Blackett, Ltd., 1903.)

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